THE PRESSURE ERRORS OF THE "CHALLENGER" THERMOMETERS[1]

II.

XI. *Accurate Measurement of Great Pressures.*

IT will be obvious from what has been said, especially as regards the old apparatus which was carried about in the *Challenger*, that one of the most essential requisites of the whole investigation was the accurate measurement of pressure. All the ordinary forms of pressure-gauge were found to be untrustworthy. It was necessary that in all cases the pressure should be measured with certainty to about 1 per cent. No attempt was made to secure any greater degree of accuracy, as the indications of the thermometers themselves could not in any case be trusted to less than 0°·1 Fahr.

The basis on which, after a great many trials, I finally founded my determination of pressures, was Amagat's[2] remarkable measurements of the volume of air and other gases at high pressures. Amagat's data were obtained in the most direct and satisfactory manner, inasmuch as he measured his pressures by means of an actual column of mercury extending sometimes to 300 metres, and more. All other means of measuring pressure are as it were valueless in comparison with this. We know by these experiments the compressibility of nitrogen, and of air, up to pressures of at least two and a half tons weight per square inch, with almost all desirable accuracy.

All that was necessary therefore in order to determine the pressures in the operating cylinder, and thus to calibrate the gauges employed, was to compress once for all a quantity of air, measure the volume to which it was compressed and the corresponding indications of the gauges, and then by the help of Amagat's tables compute the pressure actually attained. The apparatus I employed for this purpose is figured in section in the diagram below.

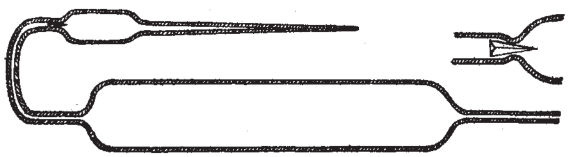

FIG. 4.—Air-gauge giving pressure (after Amagat).

This apparatus, filled with dry air, was allowed to come exactly to the temperature of the water inside the compression apparatus; then, the open lower end of it being dipped into a large vessel of mercury, it was let down full of air into the compression cylinder and pressure was applied. The effect was of course to compress the air, force up the mercury until it gradually filled the vessel and forced the air entirely into the smaller bulb. After a few trials we found roughly what amount of pressure was necessary in order just to commence the forcing of mercury into the small bulb. The mercury forced in was weighed; then the capacity of the small bulb was determined by weighing its contents in mercury. The difference of these weights is the weight of mercury, which would occupy the same volume as did the air when compressed. Finally, the original volume of the air was found by weighing the whole apparatus, first empty then filled with water; and, most important in view of Amagat's results, the barometer and thermometer were carefully observed at the instant when the apparatus had its lower end placed in the vessel of mercury. Mr. Kemp, who made these instruments for me, suggested and carried out the great improvement of inserting a small triangular pyramid of glass into the choked part of the bore (as shown in the small sketch). The effect is to break the mercury (which must be very clean) into exceedingly small drops. In this way the actual compression of the air was determined with a limit of error, represented at the utmost by the ratio of the volume of one of the small drops of mercury formed at the obstruction to the whole capacity of the small bulb. By working simultaneously with three instruments of this kind, even this very small error could be in great part eliminated:—and, practically, the compressions were measured far more accurately than was at all necessary for the purpose in hand. For greater accuracy a larger apparatus would be required. This, however, was quite unnecessary. And the requisite limit of accuracy in the experiment rendered it unnecessary to correct for the alteration of volume of the smaller bulb consequent on the pressure to which it was subjected.

In my later experiments a long carefully-gauged tube of 1·5 mm. in bore was substituted for the small bulb. This tube was coated internally with an excessively thin film of metallic silver thrown down by sugar of milk. The process was arrested the moment the film became visible by reflection. This film is at once dissolved by the mercury up to the point which it reaches at the greatest pressure, and leaves a perfectly sharp and nearly opaque edge from which to measure. This device has proved so very successful that I have now substituted it for the indices in all the pressure gauges (shortly to be described) which are employed for very accurate measures. And I am at present engaged in measuring, by comparison of a glass gauge and an air-gauge both filled in this manner, the compression of various gases at pressures up to fourfold those applied by Amagat.

XII. *Internal Pressure Gauges.*—The next step was to find some plan of construction for an instrument which, having its scale determined once for all by comparison with the air-gauge, should ever afterwards serve instead of it, thus affording a ready measure of pressure. Liquids are obviously better fitted for this purpose than solids, if only on account of their absolute homogeneity and their greater compressibility. But, unfortunately, *two* liquids must be employed, since a record must be kept:—the apparatus being surrounded on all sides by nine inches of iron:—and all my trials with two liquids were more or less unsatisfactory. The very fact that I was dealing with thermometers whose bulbs were protected from pressure, at once suggested an unprotected thermometer as something perfectly well suited to the purpose so long as the glass might be trusted to follow Hooke's law. [I have since found that the invention of such an instrument, to be used as an *élatéromètre*, is due to Parrot.[1] His investigation of the effects of pressure is wholly incorrect, as it takes no account of distortion; but the device, and the recognition of the fact that its indications are proportional to the pressure, are wholly his.]

These instruments, which, like the thermometers, are fitted with a needle-index with hairs attached, have only one defect, which is that they act like thermometers as well as pressure-gauges. That defect I managed to remove almost completely by the simple device of inclosing in the bulb a closed glass tube which *all but* fills it. The liquid then occupies only a small space between the interior tube of glass and the exterior tube forming the bulb, and is as ready as ever to give indications of pressure, while it is not in sufficient volume to be more than slightly disturbed even by a serious change of temperature.

FIG. 5.—Internal gauge, plugged.

It is quite easy, by comparing two instruments of this kind in which the ratios of the internal to the external radius of the cylindrical bulb are different, to find by trial through what range its indications are strictly proportional to the pressure. Thus all the requisites of a perfect gauge, so far as the experiments required, were met by this simple apparatus. That I have obtained a sufficient accuracy in the graduation of these instruments is proved by the close agreement between my results for the volumes of air at different pressures as measured by means of them, with the volumes corresponding to these pressures in Amagat's table. If Boyle's law had been even approximately true for these high pressures, this mode of verification would have been fallacious. It would, however, be easy to make an independent verification, by sinking some of these instruments, each thoroughly imbedded in a mass of lard (as a protection

[1] By Prof. Tait. Abridged by the Author from a forthcoming volume of the Reports of the Voyage of H.M.S. *Challenger*, by permission of the Lords Commissioners of H.M. Treasury. Continued from p. 93.

[2] "Mémoire sur la Compressibilité des Gaz à des pressions élevées," par M. E.-H. Amagat (*Ann. de Chimie et de Physique*, 1880).

[1] "Expériences de forte compression sur divers corps, par M. Parrot" (*Mémoires de l'Académie Impériale des Sciences de St. Petersbourg*, 6me. série, tome ii., 1833). The pages are headed "Parrot et Lenz," and it was by mere accident (seeking in the Royal Society's "Catalogue of Scientific Memoirs" for a reference to Lenz's thermo-electric writings) that I lit on the paper. I was much surprised at some of the statements it contains, till I found at the very end a footnote by Lenz, in which he disclaims all responsibility for the writing of the paper, and for the conclusions drawn in it.

from shocks) to a measured depth in the sea. This idea is worthy of consideration, especially if the gauge be made to register by means of a silvered tube. The only probable cause of error in such a case would be the breaking of the mercury column by a jerk, and to this all other forms are at least equally liable.

XIII. *External Pressure Gauge.*—But it was necessary not merely to measure accurately the pressure applied, but also, for the sake of the thermometers, to provide that the pressure should not be carried too far; and for that purpose it was indispensable to have an exterior indicator of pressure. This was furnished by a thin cylindrical steel tube inclosed in a cavity bored in a large block of iron, the interior of the steel tube being full of mercury and the narrow space between it and the large iron block also full of mercury. This exterior space was connected with the pressure apparatus. The pressure then throughout the whole of the space exterior to the steel cylinder was the pressure in the pump. The steel cylinder was therefore compressed from the outside. In the neck of the steel cylinder, which was screwed into the surface of the block, there was luted a vertical glass tube. It was exposed to no pressure, but the mercury in it rose, by the compression of the steel cylinder, and the height to which it rose could be easily measured. Comparative experiments were made several times by putting one of the glass gauges, whose scale had been carefully ascertained, inside the apparatus, while this newly-described gauge was also connected with it. In this way the external gauge was accurately calibrated. But, lest an accident should happen to one of the gauges, or to its index (as sometimes was the case) no experiment was made without the presence of at least three gauges. The way in which these worked together during the whole course of the experiments is the best possible proof of their value. This form of gauge, also, is greatly improved by inserting a glass tube closed at both ends into the bulb; for the temperature changes produced by pressure in mercury are greater than those in water at ordinary temperatures.

XIV. *Results of the Experiments. The True Correction for Pressure is very small.*—As soon as I applied pressure to the *Challenger* thermometers I found I reproduced pretty nearly the results obtained by Capt. Davis. I had already seen one proof that at least a large part of the result was in all probability not due directly to pressure. The experiment with the long thermometer tube showed that my theoretical calculations had been correct. The question thus became:—Is this a pressure effect of any kind; and, if so, how does it originate? and if it is not a direct pressure effect, to what is it due? There are many ways of answering such questions. One answer was furnished by one of the thermometers (A 3), whose degrees (especially on the maximum side) are very short. The whole effect (in degrees) on this thermometer was not very markedly greater for a given pressure than on the others, as it would certainly have been had the effect been entirely due to pressure directly. Another is, if it be not a direct pressure effect it must be a heating effect. With Sir Wyville Thomson's permission I got from Mr. Casella, the maker of the *Challenger* thermometers, a couple of others of exactly the same form and dimensions, but with the bulbs plugged after the manner of the gauges already described, so as to diminish their susceptibility to changes of temperature. When I put one of these into the pressure apparatus along with one of the *Challenger* thermometers, I found the effects on the new form very much smaller than on the old. Thus it was at once proved that the effect could not be due to wry-neckedness produced by the fitting on of the protecting bulb; which would have been an effect due to pressure directly; but that it must be an effect due to heat. That is to say, it was now completely established that the large results obtained by Capt. Davis are due in the main to causes which can produce no effect when the thermometers are let down gradually into the deep sea; they are due to causes connected with the thermometers, and perhaps also with the pump, but solely under the circumstances of a laboratory experiment.

XV. *Sources of the large Effect obtained in the Press.*—Now comes the question (no longer important to the *Challenger* work, but of great scientific interest), What are these various sources, and how much of the effect is due to each? First of all we have seen that the water in the press is heated when pressure is applied. Using Sir William Thomson's formula I found the amount of that heating should be about 0°·05 F. at 43° F., 0°·16 at 50°, and only 0°·3 at 59°, for one ton of pressure. [These numbers are rather too small. We do not yet know to what extent the temperature of the maximum density point of water is lowered by pressure.] These cannot be expected to be fully shown under the circumstances of the experiments, and even if they were fully shown the greatest of them represents only about one-half of the whole of Capt. Davis' result; there must therefore be some other cause. [Prof. Tait then gives details of the various experiments by which he traced the sources of the large effect obtained.]

Thus it appears that there are no less than five different causes which contribute each its share to Capt. Davis' result. Of these, one is independent of the others, and would produce its full effect even if they were not present. The other four give effects which are not cumulative, and it would be very troublesome to try to assign to each its exact share of the result when two or more act together. Fortunately, it will be seen that we do not require to attempt to solve this problem.

(1.) First is the direct effect of the external pressure upon the exposed part of the thermometer tubes. This, in general, will be found very small, except in tubes where there are large aneurisms. The whole effect of 3 tons pressure on a *Challenger* thermometer without aneurisms, at temperatures near freezing point, so far as the minimum index is concerned, would be only about 3 one-thousandths of 30 degrees or so, that is 90 thousandths or at most 0·1 of a degree for 3 tons pressure. That is an amount which, in consequence of the necessary errors of reading the thermometers, may be entirely neglected, and, unless there are large aneurisms, there will be little need for pressure corrections even in six miles of sea.

The other parts of the observed effect were

(2.) Heating of water. This I observed to follow very nearly, according to Thomson's formula, the original temperature of the water. By comparing the pressure effects on the same thermometers during summer, and during winter (for which latter the late continued frost was of particular service, and enabled me to work for many days at the temperature of the maximum density of water), I found the results to vary in accordance with calculation.

(3.) Heat due to friction during pumping. This from its very nature was unavoidable unless we could have got an apparatus into which (by enormous pressure) the plug could have been forced directly. This could not, however, have been done in my laboratory, even if the apparatus had been adapted to such a form of experiment. But it was very easy to calculate the extreme possible amount of this effect.

(4.) The peculiar heating effect due to the vulcanite mounting. I verified this effect of vulcanite by taking a thermometer which had no vulcanite about it and measuring the effect produced upon it by a definite pressure, and then putting loosely round the bulb (in a test-tube, which had itself been previously experimented on) a small quantity of vulcanite in thin plates. I found that so little as 8 grammes of vulcanite round the protecting bulb raised the effect produced by a pressure of 3·2 tons weight from 0°·5 F. to 1°·1 F. The vulcanite was in thin strips about a millimetre and a half in thickness. The effect of the vulcanite on the *Challenger* thermometers (in the hydrostatic press) must, from the mode of their construction and mounting, in all cases be considerably greater than this.

Under these circumstances, we might without farther inquiry fairly attribute the whole outstanding effects to the massive vulcanite slabs on which these thermometers are framed. But there still remains

(5.) The most difficult question of all, the temperature effect produced by pressure upon the protecting bulb, which is under different circumstances altogether from the vulcanite; for the vulcanite is simply compressed, while the glass sheath is under pressure on one side and not on another, and is therefore subject to shear as well. In its interior the glass is extended in a radial and compressed in a tangential direction. Nobody has yet made any approximation to an answer to the question what effect in the way of heating or cooling will be produced by deformation which consists partly of compression and partly of change of form. We know that in indiarubber a cooling effect is produced by traction, and it may happen that a similar change of form in glass also produces a reduction of temperature. This is a question, however, which is not capable of answer by the help of my present apparatus;—though it will probably be answered by experiment before theory is able to touch it. The results of my experiments on the thermometers with plugged bulbs show that, on the whole, a heating effect results from the combined compression and shear in a bulb exposed to external pressure only. This has been verified by cutting down a thermometer, an exact counterpart of the *Challenger* thermometers

but without aneurisms, taking out the greater part of the mercury and inserting a second (now a maximum) index in the minimum side of the tube. When this instrument was stripped of its vulcanite, the effect of pressure at 40° Fahr. was considerably greater than that due to compression of the tube.

But it does not require to be taken into account so far as the *Challenger* thermometers are concerned.

XVI. *Final Conclusion from the Investigation.*—The final conclusion is that only one of these five causes, which are active in the laboratory experiment, can affect the *Challenger* thermometers when let down into the sea, namely, pressure. There is there no heating of water by compression; there is no heating by pumping; there is no heating of vulcanite, because the thermometers are let down so quickly in comparison with the rate of increase of pressure that each little rise of temperature is at once done away with as the thermometer passes through a few additional yards of water; and the effect on the protecting glass also, for the same reason, which is a heating effect on the whole, is all but done away with step by step as it is produced. All these four causes, therefore, which made Capt. Davis' correction so much too large, are valid only for experiments in a laboratory press, and not for experiments in the deep sea. Therefore, as a final conclusion, I assert that, if the *Challenger* thermometers had had no aneurisms, the amount of correction to be applied to the minimum index would have been somewhat less than 0°·05 F. for every ton of pressure, *i.e.* for every mile of depth. All the thermometers which have large aneurisms have had special calculations made for them, but in no case does the correction to be applied to the minimum index exceed 0°·14 or about one-seventh of a degree per mile of depth.

[From] the *Appendices* to Prof. Tait's Report, which contain numerous formulæ with detailed descriptions of apparatus and modes of experimenting, we make the few following extracts.]

The diminution per unit volume of the interior of a cylinder with closed ends, of internal radius a_0, and external radius a_1, when exposed to an external pressure Π, is

$$\Pi \frac{a_1^2}{a_1^2 - a_0^2}\left(\frac{1}{n} + \frac{1}{k}\right).$$

Here n is the rigidity, and $\frac{1}{k}$ the compressibility, of the walls of the cylinder.

When Π is a ton-weight per square inch, the value of the quantity

$$\Pi\left(\frac{1}{n} + \frac{1}{k}\right),$$

is, according to the best determinations, somewhere about $\frac{1}{1000}$ for ordinary specimens of flint glass, and about $\frac{1}{4000}$ for steel. This expression is very simple, and enables us at once to calculate the requisite length of bulb, when its internal and external radii are known, which shall have any assigned sensitiveness when fitted with a fine tube of a given bore. To obtain great sensitiveness, increasing the diameter of the bulb is preferable to diminishing its thickness, as we thus preserve its strength; and we have seen how to avoid the complication of temperature corrections.

As a verification of this formula, in addition to the simple one described in the text above, I had an apparatus constructed of ordinary lead glass of the following dimensions:—Length of cylindrical bulb, 745 mm. Ratio $a_0 : a_1 = 8·7 : 21·9$. The weight of mercury filling 424 mm. of this bulb was 167 grm. To the bulb was attached a smaller tube of which the mercury filling 68 mm. weighed 1·43 grm.

Hence we have

$$\frac{a_1^2}{a_1^2 - a_0^2} = 1·187.$$

Also the content of the whole bulb in mercury is $\frac{745}{424}167$ grm. = 293·4 grm. Hence a pressure of one ton-weight should force into the narrow tube $\left(\frac{1·187}{1000}293·4 = \right)$ 0·348 grm. of mercury.

This ought to displace the index through $\left(\frac{0·348}{1·43}68 = \right)16^{\text{mm}}·55$.

Comparing this with the result of experiment, we had the following remarkably satisfactory numbers:—

Tons.	Calculated.	Observed.
0·9	14·9	14·6
1·4	23·1	21·2
3·1	51·3	48·9

There was no glass tube in the interior of the bulb, so that the slight discrepancies between the ratios of calculated to observed effects are mainly due to effects of temperature.

In the *Proc.* R.S., June, 1857, Sir William Thomson gives for the rise of temperature of a fluid, the pressure on which is suddenly raised from p to $p+\omega$, the general expression

$$\frac{te}{\text{JK}}\omega.$$

Here t is the absolute temperature of the fluid; e its coefficient of expansion, and K its average capacity for heat, under constant pressure, between p and $p+\omega$. J is Joule's equivalent.

The value of e, as given by Kopp's experiments, is nearly

$$\frac{t-278}{72,000},$$

for temperatures within 20° C. of the maximum density point. The mean of the experimental determinations of Matthiessen, Pierre, and Hagen, makes it about 5 or 6 per cent. greater.

For the Centigrade scale the value of J is 1390 foot-lbs. An atmosphere of pressure is nearly 2117 lbs. weight per square foot; and K is about 63·45 (the number of pounds of water in a cubic foot).

Hence it follows that, for one additional atmosphere of pressure, the temperature of water is raised (in degrees Centigrade) by about

$$\frac{t(t-278)}{2,850,000}.$$

Now 56° F. is 13°·3 C., for which $t=287·3$, and the rise of temperature produced by a ton-weight per square inch is

0°·14 C. or 0°·25 F.

This is the statement in the text.

From the above formula we find the heating effect of one ton pressure on water at 50° F. to be nearly

0°·16 F.;

and for each degree above or below 50° F. this number must be increased or diminished by about one-tenth of its amount.

This expression is very easy to recollect, and it gives the results with ample accuracy throughout the whole range of temperatures (40°–60° F.) within which my experiments were conducted.

It is to be observed that Thomson's formula is strictly true for small pressures only. No account has been taken of a possible lowering of the temperature of maximum density, or of a change of expansibility, under pressure. Nor is it known how a considerable increase of pressure affects the thermal capacity.

On the first occasion on which one of the thermometers gave way, we were much surprised at the loudness and musical quality of the sound produced. The whole mass of iron and steel vibrated like a bell in consequence of the (comparatively slight) sudden relaxation of pressure. On another occasion, just as a pressure of three and a half tons had been reached, the whole apparatus gave a strong, protracted musical sound, which continued until the screw-tap was opened. This was probably due to a species of hydraulic-ram behaviour on the part of one of the valves of the pump. These are little conical pieces of steel, with the points much elongated, which are ground accurately into conical beds, and fall back into their places by gravity. It was not observed that this powerful vibration had in the least degree altered the position of the indices in the thermometers or gauges which were in the pressure chamber. Their indications agreed perfectly with those of the preceding and succeeding day.

I made a number of experiments with the view of determining the amount of distortion at which glass gives way, with the view of finding the limit of strength of a glass tube, and also the ratio of external to internal diameter to secure it against any assigned lower pressure. I allude to them now in consequence of a curious fact observed, which gives the explanation of a singular occurrence noticed on board the *Challenger*. The walls of the tubes, when they gave way, were crushed into fine powder, which gave a milky appearance to the water in the compression apparatus. But the fragments of the ends were larger, and gave much annoyance by preventing the valves of the apparatus from closing. To remedy this inconvenience, I inclosed the glass tube in a tube of stout brass, closed at the bottom only, but was surprised to find that it was crushed almost flat on the first trial. This was evidently due to the fact that water is compressible, and therefore the relaxation of pressure (produced by the break-

ing of the glass tube) takes time to travel from the inside to the outside of the brass tube; so that for about 1-100ooth of a second that tube was exposed to a pressure of four or five tons weight per square inch on its outer surface, and no pressure on the inner. The impulsive pressure on the bottom of the tube projected it upwards, so that it stuck in the tallow which fills the hollow of the steel-plug. Even a piece of gun-barrel, which I substituted for the brass tube, was cracked, and an iron disk, tightly screwed into the bottom of it to close it, was blown in. I have since used a portion of a thicker gun-barrel, and have had the end welded in. But I feel sure that an impulsive pressure of ten or twelve tons weight would seriously damage even this. These remarks seem to be of some interest on several grounds, for they not only explain the crushing of the open copper cases of those of the *Challenger* thermometers which gave way at the bottom of the sea, but they also give a hint explanatory of the very remarkable effects of dynamite and other explosives when fired in the open air.

To show how possible is a serious mistake in the measurement of pressure, I append a comparison of the indications of the very elaborate gauge attached to the old *Challenger* apparatus with those of my steel external gauge already described. The scale of the *Challenger* gauge is divided to cwts. on the square inch. My gauge gives very nearly 20 mm. per ton; so that, for a rough comparison, we may take 1 mm. as equivalent to 1 cwt. The two instruments were simultaneously attached to the pump, and the pressure was therefore the same in both at each reading. There can be no doubt whatever, from repeated comparisons with glass gauges of all sizes and shapes, that my gauge follows Hooke's law with great accuracy. The only possibility of serious error is in the actual value of the unit. This important determination has, however, been very carefully repeated by the aid of Amagat's numbers and the indications of the silvered gauge already described; and the result is as above stated.

Steel Gauge. Millimetres.	*Challenger* Gauge. Cwts. per sq. in.	Ratio.
0	0	...
5	0	0·0
9	1·2	0·13
15	8·7	0·58
20	13·9	0·69
30	23·6	0·78
40	35·0	0·87
50	47·0	0·94
60	58·7	0·98
70	71·7	1·02

The comparison was repeated several times with almost exactly the same results.

It is quite clear that the *Challenger* gauge does not follow Hooke's law. It lags behind the steel gauge at first (does not give any indication, in fact, till the pressure is nearly 50 atmospheres), then gradually gains on it; and, at pressures greater than 3½ tons, appears to leave it rapidly behind. The instrument is, however, graduated up to 4 tons only. My very first experiments with this *Challenger* instrument, in which I used a simple form of manometer, showed that it was not trustworthy, and led me to make various trials for the purpose of getting a proper mode of measuring high pressures.

Finally, it may be interesting to mention that a fairly approximate determination of the compressibility of water was made by counting the number of strokes of the pump required to produce a measured pressure in the interior of the large apparatus.

[Then follows a table of the experimental data for each of a large series of the *Challenger* thermometers. These are of no general interest. Their importance is confined to the reduction of the actual observations made on board the *Challenger*].

THE GRASS BARRIERS OF THE NILE

THIS interesting phenomenon, which so largely contributes to produce changes in the bed of the Nile and to accumulate river formations of great geological importance, has been recently investigated by M. Ernest Marno, who has just published an elaborate paper on the subject, in the last number of *Petermann's Mittheilungen*. It is accompanied by a map, on the scale of 1 to 500,000, of the Bahr-el-Gebel and of the Bahr-el-Abiad, from Geseir Abbas to Sohat, and of the Bahr-el-Serat from its mouth to 7° 30′ N lat. After having made its way among the hilly region, through several great lakes, formerly forming a series of terraces and connected together by short rivers, the Nile, or the Bahr-el-Gebel—the River of the Mountains—enters an extensive flat land, which it crosses over six degrees of latitude to the next rocky barrier, which it cuts through at Khartum. Over this stretch it runs with numerous windings, first north to its confluence with the Bahr-el-Ghazal, and then to the east, under the name of Bahr-el-Abiad, and, although the direct distance between its issue from the hilly tract to Khartum is only 600 miles, the total length of the river with its windings is no less than 1100 miles. The whole of this region is a wide marsh, and the river has no proper banks, its water being mixed with that of marshes which cover the whole of this tract. It is even a rare occurrence to see dry banks, as the country is more like an extensive marsh, through the midst of which a somewhat deeper channel has been dug by the current of the river. Numerous smaller rivers connected together and with the main channel and its numerous ramifications circulate amidst these marshes, and during the rainy season the *maije*, or lateral ponds and lakes, increase yet more, covering wide tracts of land, whilst during the dry season some stretches of banks re-appear, and the lakes which were navigated by steamers some months before become simple marshes. Vegetation plays an important part in the modifications which are going on in this region. The country is covered with rich grasses, mostly consisting of such species (*Saccharum spontaneum*, *S. irschæmum*, *Vossia*) as grow perfectly well even in water; this grass can be lifted with its roots by water, and grow floating on the surface, so as to render it most difficult to draw a line of demarcation between land and water. Thick and high papyrus palms grow sometimes on the very banks of the main channels of circulation of the water, and strengthen these by their complicated roots, but they do not cover all the banks, and the outlines of the river are mostly indefinite. Some few tree-like *Herminiera elaphrosylon* grow as isolated individuals on the banks of the rivers, and of the *maije*, whilst the smaller marshy and aquatic flora (*Pistiæ*, *Nymphea*, *Vallisneriæ*) nearly disappears in comparison with the rich vegetation of the above-named species. The fauna of this region closely depends upon the season. Mammals and birds leave it during the rainy period and wander to the hilly tracts, but during the dry season the banks of the *maije* and of the rivers are peopled with elephants, buffaloes, giraffes, antelopes, and by many kinds of birds. Besides this region has also its special forms, namely the *Balæniceps Rex*, the *Protopterus æthiopicus*, and the ganoid fish, *Polypterus*, all being remains from earlier geological periods. The people who inhabit this region, the Dinka, the Shilluk, and the Nuehr, all belong to a very low level of civilisation, living mostly on their herds of cattle; they change their abodes in accordance with the season, but they cannot be considered as true nomades, as the land occupied by each tribe is strictly limited by other tribes, and every encroachment on another's land is punished by war.

It is obvious that in this region the fall of the rivers is very small and that the regular outflow of water may be checked by winds and other occasional circumstances; whilst the great quantities of water poured down into the basin during the rainy season cannot find an easy way through the flat channels; extensive inundations occur therefore every year, and when the rains are especially heavy, great masses of floating grass are brought from the *maije* into the main river, and accumulate in its windings. New floating islands of grass are brought by and by to these barriers, being pressed upon or beneath them, and soon the whole of the river throughout its width and depth is obstructed by these barriers, which the inhabitants call *setts*. The grass does not decay in